

**Does Geology Matter to Streamflow? A
geo-climatic assessment of spatial variability
in summer flow sensitivity to climate change
for San Francisco Bay – Delta system**

Rachel A Cook

Public Comments

No public comments were received for this proposal.

Initial Selection Panel Review

Proposal Title

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatial variability in summer flow sensitivity to climate change for San Francisco Bay – Delta system

Funding:

Do not fund

Initial Selection Panel (Primary) Review

Topic Areas

- Implications Of Future Change On Regional Hydrology, Water Operations, And Environmental Processes

Please describe the relevance and strategic importance of this proposal in the context of this PSP. How does the proposal address the topic areas identified above? What are the broader CALFED Goals this proposal may meet that are not accounted for in these specific topic areas?

This proposal addresses the prevailing consensus that California's future climate will feature warmer temperatures, more rainfall and runoff during winter rather than snowpack accumulation, and drier summers and falls leading to water shortages during the growing season. The PIs describe clearly the hypothesis that these conditions will be exacerbated in those mountain regions of the central and southern Sierra Nevada underlain by granitic rock with very limited groundwater storage capacity, and moderated in those regions to the north that are underlain by volcanic rock with much greater storage capacity. The objective of the study is to quantify, largely through modeling, the importance of geologic variation in characterizing the spatial pattern of hydrologic response to climatic variability. Thus, the proposed work addresses the key issue of the effects of future change on regional hydrology.

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Initial Selection Panel Review

The budgets of proposals submitted in response to this PSP are larger, on average, than those submitted to CALFED in previous years. The Science Program is committed to getting as much science per dollar as is reasonably possible. With this commitment in mind, can the proposed budget be streamlined? If so, please recommend and clearly justify a new budget total in the space provided.

The overall reaction of the reviewers is that the budget is reasonable given the number of individuals working on the project, but that there is some question (particularly from one reviewer) about whether all of the proposed effort will greatly increase the practical understanding (e.g., from the perspective of water managers) of the processes being studied over the general understanding already in hand. In other words, the PIs have done a good job in describing the differences between the two regimes in terms of response to flows. But, limiting the study to sites representing the two extremes (Merced River and Hat Creek) and two intermediate sites (Cole Creek and Duncan Canyon Creek) without extrapolation of the results to address broad scale responses to climate change is perhaps a missed opportunity to make a more substantial contribution. A primary conclusion of the Technical Synthesis Panel was that "broad-scale hydrogeologic response to climate change might be better addressed through more spatially extensive simulations using the authors' prior research results, rather than focused detailed work in a limited number of sites.

Evaluation Summary And Rating.

Provide a brief explanation of your summary rating and any additional comments you feel are pertinent.

All reviewers agree that this project, taking advantage of considerable skills and experience of the PIs, will result in very useful advancements in our understanding of the relationships between geology and climate change in determining the storage capacity of different parts of the Sierra Nevada. The question is whether the payoff for water management will be the achievement of new insights or simply more detailed information about processes that are already qualitatively appreciated.

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Selection Panel (Discussion) Review

fund this amount: \$0

note:

do not fund

This proposal was very well-written and documented. The Panel felt that this proposal would contribute information to our knowledge base but that the marginal increase in our practical hydrological understanding provided by this project would not justify its expense. The Panel believed that we already know a substantial amount about the linkages that would be addressed here - more than we know about other substantial processes and linkages in the watershed for this Estuary. Also, the Panel believed that the spatial extent of this study was probably too narrow to provide significant breakthroughs in our understanding of the system as a whole. Finally, since the studies are targeted towards watersheds that are behind dams, the hydrological implications of climate change (timing and magnitude of flow) discovered in this study will be attenuated, to some degree, by dam operations.

Panel Ranking: Do not fund

Technical Synthesis Panel Review

Proposal Title

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatial variability in summer flow sensitivity to climate change for San Francisco Bay – Delta system

Final Panel Rating
above average

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: The proposal addresses an important topic of how local geology may mediate hydrologic response to climate change through soil characteristics (thickness, conductivity, etc.) and thereby affect run-off rates and water quality. It is a strong proposal with very capable PIs that have considerable prior experience, and will provide a useful product for resource managers and CALFED. Primary criticisms: •Hypothesized effects of geology already qualitatively known, so primary product will be quantification and confirmation of these effects. This, nevertheless, is a valuable product. •Geologic variability simplified to two cases (northern volcanics vs. central granites) that will extend/refine authors' previous studies of Oregon Cascades, but may not produce substantially new insight. Will be new for California, though. •Limited number of study sites and limited geologies may limit regional extrapolation of results/assessments. Nevertheless, is a valuable pilot study. •Methods for the empirical portion of the study are at times vague (e.g., what length of hydrologic and meteorologic record will be examined, how will the isotopic analysis of drainage efficiency be conducted, etc.). The technical reviewers overall ratings of this proposal were good, excellent, and very good (very good on average, agreeing with the rating given here (above

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Technical Synthesis Panel Review

average)). Goals: Clear, consistent and timely. The authors recognize that regional changes in the timing and style of precipitation are primary controls on hydrologic response to climate change, but hypothesize that differences in geology and groundwater flow may play an important mediating role. One reviewer questions the significance of the project (effects of geology qualitatively known), but I would argue that further quantification and prediction of response to climate change are needed and would be valuable to the state. Justification: The study is well justified, as discussed above, and a conceptual model is provided and linked to a detailed physical model (Figs. 2 & 7). Hypotheses are explained and justified using prototypical data analyses. Approach: Methods for the physical model (RHESSys) are well described and have been employed in prior studies by the authors, but methods for the empirical portion of the study are at times vague (e.g., what length of hydrologic and meteorologic record will be examined, how will the isotopic analysis of drainage efficiency be conducted, etc.). Other important criticisms: geologic variability simplified to two cases (northern volcanics vs. central granites) that will extend/refine authors' previous studies of Oregon Cascades, but may not produce substantially new insight; and limited number of study sites and limited geologies may limit regional extrapolation of results/assessments. Feasibility: The scope of work is well within the PIs' range of experience and expertise. However, the empirical approach is not fully documented, making it difficult to assess the feasibility of that portion of the project. The physical model is better described and has a high probability of success. Monitoring: Not applicable. Products: Knowledge transfer/data dissemination could be strengthened. Publication and reporting are not well discussed, but are expected from the PIs. GIS-based visualization is proposed, but details of the actual product are vague. Nevertheless, the primary product (i.e., improved understanding of the hydrologic role of geology) will assist resource managers in planning for climate change and better predicting consequent spatial changes in water resources for the state. Capabilities: The PIs are all highly qualified for their tasks and have considerable prior experience using the proposed methods and analyses. Budget: Budget seems reasonable for the

Technical Synthesis Panel Review

time frame of the project, although items questioned by Reviewer 3 should be examined by CALFED.

Additional Comments:

Summary: The proposal addresses an important topic of how local geology may mediate hydrologic response to climate change through soil characteristics (thickness, conductivity, etc.) and thereby affect run-off rates and water quality. It is a strong proposal with very capable PIs that have considerable prior experience, and will provide a useful product for resource managers and CALFED. Primary criticisms: •Hypothesized effects of geology already qualitatively known, so primary product will be quantification and confirmation of these effects. This, nevertheless, is a valuable product. •Geologic variability simplified to two cases (northern volcanics vs. central granites) that will extend/refine authors' previous studies of Oregon Cascades, but may not produce substantially new insight. Will be new for California, though. •Limited number of study sites and limited geologies may limit regional extrapolation of results/assessments. Nevertheless, is a valuable pilot study. •Methods for the empirical portion of the study are at times vague (e.g., what length of hydrologic and meteorologic record will be examined, how will the isotopic analysis of drainage efficiency be conducted, etc.). The technical reviewers overall ratings of this proposal were good, excellent, and very good (very good on average, agreeing with the rating given here (above average)). Goals: Clear, consistent and timely. The authors recognize that regional changes in the timing and style of precipitation are primary controls on hydrologic response to climate change, but hypothesize that differences in geology and groundwater flow may play an important mediating role. One reviewer questions the significance of the project (effects of geology qualitatively known), but I would argue that further quantification and prediction of response to climate change are needed and would be valuable to the state. Justification: The study is well justified, as discussed above, and a conceptual model is provided and linked to a detailed physical model (Figs. 2 & 7). Hypotheses are explained and justified

Technical Synthesis Panel Review

using prototypical data analyses. Approach: Methods for the physical model (RHESSys) are well described and have been employed in prior studies by the authors, but methods for the empirical portion of the study are at times vague (e.g., what length of hydrologic and meteorologic record will be examined, how will the isotopic analysis of drainage efficiency be conducted, etc.). Other important criticisms: geologic variability simplified to two cases (northern volcanics vs. central granites) that will extend/refine authors' previous studies of Oregon Cascades, but may not produce substantially new insight; and limited number of study sites and limited geologies may limit regional extrapolation of results/assessments. Feasibility: The scope of work is well within the PIs' range of experience and expertise. However, the empirical approach is not fully documented, making it difficult to assess the feasibility of that portion of the project. The physical model is better described and has a high probability of success. Monitoring: Not applicable. Products: Knowledge transfer/data dissemination could be strengthened. Publication and reporting are not well discussed, but are expected from the PIs. GIS-based visualization is proposed, but details of the actual product are vague. Nevertheless, the primary product (i.e., improved understanding of the hydrologic role of geology) will assist resource managers in planning for climate change and better predicting consequent spatial changes in water resources for the state. Capabilities: The PIs are all highly qualified for their tasks and have considerable prior experience using the proposed methods and analyses. Budget: Budget seems reasonable for the time frame of the project, although items questioned by Reviewer 3 should be examined by CALFED.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The Panel agreed with the Primary review. The proposed work is a useful pilot, but the limited number of sites and lithologies are insufficient for general extrapolation of results to address broadscale response to climate change. Although the panel was generally supportive of the project and

Technical Synthesis Panel Review

goals, the panel members suggested that broadscale hydrogeologic response to climate change might be better addressed through more spatially extensive simulations using the authors prior research results, rather than focused detailed work in a limited number of sites.

Rating: Above average

Technical Review #1

proposal title: Does Geology Matter to Streamflow? A geo-climatic assessment of spatial variability in summer flow sensitivity to climate change for San Francisco Bay – Delta system

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>Yes, the goals, objectives and hypotheses were all clearly stated and internally consistent.</p> <p>The central idea upon which the goals, objective, and hypotheses are focused is that differences in stream flow regimes, and their potential response to climate change, is significantly influenced by geology (ie North-eastern California volcanic-based drainage basins and the remaining granitic-based basins stream flow regimes are different largely due to the greater water-holding capacity of the volcanic vs. granitic bedrock).</p> <p>Is the idea timely and important? From my understanding, the most important control on stream response is the rainfall intensity, especially in this part of the world (rain-on-snow events). That places geographic constraints on the importance of the research, but I don't count that against the proposal, since California is clearly the location of interest for this RFP. Groundwater flow will always adjust to accommodate a non steady-state condition (ie changes in precipitation forms and seasonal distribution, changes in snowmelt dates, etc). Also, I didn't see the need to spend so much effort on modeling this response, given both the difficulty in quantifying the geologically-controlled water-holding capacity, and</p>
----------	---

Technical Review #1

	<p>the fact that the already observed differences in stream flow already tell us about the different systems. Don't we know the geological controls on baseflow already just by looking at the hydrographs? For these reasons, I don't see the importance of this proposed research.</p> <p>I would rate this section as "G". The goals, objectives, and hypotheses, are clear and consistent, but I don't see the research as really important to help assess the impacts of future climate change on the Bay-Delta hydrological system.</p>
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>I don't think the study is justified relative to existing knowledge. I think most realize that baseflow is a function the release of water from groundwater sources (deep subsurface flow), and therefore (with all other variables held constant) will vary with contrasting controls such as the porosity and hydraulic conductivity of the bedrock, and the imposed hydraulic gradient. The authors show this in Figure. 3, for example. The proposal is essentially a modeling study, with a small field component, so what is the point of modeling these basins when the answers to the proposed questions are already known? The authors will argue that the point to model the two different systems (volcanic vs. granitic) is to determine the possible effects of climate change. Again, I believe the answer to this question is already known in the literature.</p> <p>A conceptual model was clearly presented (eg. Figures 2 and 7) that explained, albeit in very simple terms,</p>
----------	---

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Technical Review #1

	the underlying basis for the proposal. A flow chart and/or conceptual model of the RHESSys showing how the simulated climatological changes would be coupled to RHESSys and the different basis would have been helpful. As mentioned before, I don't think that full-scale implementation of the project is justified based on the already existing knowledge of difference in baseflow in the region, and lack of justification for detailed modeling of such systems based on the errors and difficulties associated with parameterizing groundwater flow parameters. It was mentioned that the empirical portion of the study will help with this parameterization (and I agree), yet I believe the existing observations have already answers the proposed questions better than a model could.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Overall, the approach is feasible, yet not well-designed for meeting the objectives. This was largely the result of missing details throughout the proposal. For example, in the "Empirical Time Series Analyses" section: What does "long-term streamflow records" mean? How long? What periods? "...for a larger set of sites in the northern Sierras...long-term..." Which sites, and again, what periods? "Snow pillow data..." An important source of data in this region, but no discussion at all was presented, just a url link. "Drainage efficiency...derived from isotopic analysis..." Again, no methodological details were presented on this critical aspect. As these examples show, the empirical aspects approach was not well presented or described, leading one to question how the subsequent modeling exercise could be improved by this analysis.
----------	--

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Technical Review #1

	As already mentioned, I do not believe that this project will generate a large volume of novel information. The methods, especially the empirical analyses, were scantily described, and the use of a model is standard. This information, however, could be useful to decision makers if they don't know the basic stream flow responses already.
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach was not fully documented. Important details were missing, especially in the "empirical time series analyses", as described above. The modeling section was better explained, but here too there were few details on how the groundwater aspect of the model will be dealt with and integrated with the results from the empirical time-series analyses. Constant referencing to other works, the use of only one equation, and the absence of a flow chart or similar schematic of the basics of the model makes it hard for a reviewer to judge the feasibility of this approach, hence the likelihood of success.
Rating	poor

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The experimental design is essentially a pair-plot study (1 stream-volcanic; 1 stream-granitic, times two), with plans to interpret historical stream flow data, but the periods of observations were not given. A model will be used to simulate the effects of
----------	--

Technical Review #1

	simulated climate change on stream flow. Therefore, caution must be used, since one model is driving another.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The product is likely the calibration of an existing model for the four rivers. The model, in turn, could be used as an additional tool to help understand the hydrology of the region.
Rating	fair

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The PI appears to have extensive experience with the model they plan to use. The CO-PI's were individually assigned to cover the empirical stream flow analyses (Kirchner), the "geological framework" (I'm not sure what that exactly means) (Grant), and the groundwater modeling (Magna). All are productive scholars in their respective areas, but Kirchner's extensive work seems to be somewhat of a miss-match of expertise with his assigned task on this project. A graduate student researcher, two computer technicians, graduate research assistant were also listed on the project. Appears to be a heavily-staffed project without much
----------	--

#0245: Does Geology Matter to Streamflow? A geo-climatic assessment of spatia...

Technical Review #1

	field work (Nine project staff, in total).
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Most (nearly all) of the budget items are for salaries, wages, benefits, and tuition reimbursements. None of these amounts appear unreasonable, yet with nine staff on the project, the budget quickly reaches over \$550,000 for a three-year study.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>My summary rating for each category is summarized in the table below. I calculated my overall rating as: $3+2+2+1+3+2+5+4 = 22/40$, equivalent to 2.75/5.00 (based on "Excellent" = 5... "Poor" = 1), which rounds to a "Good" on your scale.</p> <p>Category Rating Summary Comment Goals G Goals/Objectives clear, but I question the importance. Justification F Not clear why we need to model this, or how this will help with the interpretation of existing observations. Approach F Serious deficiencies. Lack of details and adequate descriptions of the methodology. Feasibility P Hard to judge feasibly when the approach isn't clear. Monitoring G "Monitoring" is via model output. Products F Calibration of an existing model can be if some use, but not clear how this will enhance the examination of existing long-term data. Capabilities E A large and capable team. Budget VG Budget not unreasonable, but large for a model study due to nine</p>
----------	--

Technical Review #1

	staff.
Rating	good

Technical Review #2

proposal title: Does Geology Matter to Streamflow? A geo-climatic assessment of spatial variability in summer flow sensitivity to climate change for San Francisco Bay – Delta system

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals, objectives, and hypotheses for the most are clearly stated and internally consistent. The proposal title and some of the introductory statements imply a broader scale examination of the effects of geology and streamflow than the project proposal actually indicates. The idea is timely and important in the context of growing concerns about climate change and the need to adequately model and plan for associated impacts.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The authors make a strong case that a) geology affects streamflow and that b) existing models of climate effects on streamflow do not adequately incorporate such effects. The authors do not as convincingly show how their work will translate into improved planning. The authors clearly explain a conceptual model of how geologic variations may affect
----------	---

Technical Review #2

	<p>streamflow responses to changes in precipitation, as may occur due to global warming. A substantial portion of the proposed research is devoted to elaborating on and quantifying the qualitative conceptual model described by the authors for how geology affects streamflow in the Sierra Nevada. The authors then suggest that they will use the improved understanding they have gained to model how climate change effects on streamflow will vary with geology. But, the authors do not clearly indicate how they will elaborate results beyond their study areas, and how they will communicate results to planners, as discussed further below. The authors argue that the proposed work is needed to make future water resources planning associated with climate change more sophisticated by incorporating geologic effects on streamflow, but more directed efforts by the proposal authors to achieve this is merited.</p>
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The components of the study (empirical analysis, groundwater modeling, physically based modeling using RHESSys, and climate scenario modeling) build nicely on each other and will likely contribute to more detailed understanding of how broad differences in geology affects streamflow dynamics and responses to changing precipitation. Because a fairly strong understanding of how geology affects streamflow has already been developed by the authors, primarily based on their work in Oregon, the new insights produced</p>
-----------------	---

Technical Review #2

here are more likely to provide further refinement and quantification of how geology affects streamflow than broad new insights.

This study proposes to examine how geology affect streamflows in the Bay Delta system, but the proposed research appears to examine only two broad types of geologies, volcanic and granitic. What percent of contributing area to Bay-Delta system do these represent? Are there other geologic units draining to the Bay Delta (e.g., areas draining west side of Sacramento/San Joaquin basins, or Southern Sierra) that might behave differently than the 2 geologic types that will be examined here? Based on the proposal title, I was expecting a more comprehensive assessment of geologic effects. The authors state in the introduction (p.4) that a broader range of geologic terrains are present in the Sierra than in western Oregon, where several of the PI's have previously worked. But the approach here resembles research Tague (lead PI) previously conducted in Oregon, with northern versus southern Sierra substituting for High versus Western Cascades.

Certain components of the approach are vague. The section on empirical time series analysis suggests that the response of different geologies to climate trends will be evaluated using streamflow records. It is not clear, however, whether the broad geologic stratification of volcanic versus granitic will be used here (i.e., only two types of geologies), or whether more detailed geologic stratification will be evaluated. The empirical analysis section also suggests that long-term gauging records will be evaluated. Will this include an investigation of streamflow responses to climate change that are already evident, and of how these differ based on geology? The description of the analysis of drainage efficiency using isotopic analysis (page 14) is also vague.

Technical Review #2

	<p>The section on spring system groundwater (sloppy to abbreviate this word in the section heading on page 14) modeling states that the response of large volume springs in Northern California to future climate change will be quantified. Earlier this study component has suggested a focus on the Hat Creek system. Is the goal to quantify the response in the Hat Creek system only, or more broadly? How representative is Hat Creek, and how will modeling results based on Hat Creek be transferred elsewhere? How will these results be integrated to illustrate broad effects in Northern California? More detail on the 2 climate scenarios to be examined here would also be useful. Are both scenarios for a 2*CO2 future, or for some higher or lower emissions scenario? Given CalFed's concern for understanding potential warming impacts, modeling of a broader range of climate scenarios may be merited here.</p> <p>One of the hypotheses proposed here is that climate change will have its greatest effect on summer streamflows in intermediate elevation watersheds. This hypothesis is not clearly addressed in this proposal, and it is unclear what the implications of this would be for broader scale water resources planning in the Bay-Delta system (ecological implications in mid-elevation streams may be important but are not part of the project proposed here). This hypothesis is also unrelated to the broad proposal topic of how geologic affects streamflow. The authors suggest that the empirical time series analysis will consider elevation effects, but did not explain how this will be tied back into the broader questions of geology-streamflow relationships, climate change effects and implications for water planning.</p>
Rating	good

Technical Review #2

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The approach is technically feasible, and there is a high likelihood that the basic project objectives will be achieved. Certain aspects of the approach are not clearly documented, as discussed above. The scale of the project in some respects falls short of the broadly stated objectives of understanding how geology affects streamflow and implications of this for climate change in the Bay Delta system, as discussed above. Much of the project involves application of methods or models that the authors have previously used in other settings and/or that the authors have developed themselves, suggesting that the scale of the project is well within the grasp of the authors.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	No monitoring is proposed.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The proposed research will produce a more sophisticated understanding of relationships between streamflow, geology and climate in the Sierra Nevada, and will likely result in associated scientific
-----------------	--

Technical Review #2

	<p>publications. There is potential for this research to contribute to water resources planning efforts in California, although the proposal does not adequately articulate how such contributions and communications will occur. Communication of results to planners is an extremely important component of any sort of "impacts modeling" type research, as is proposed here. The authors propose a "GIS-based dynamic visualization of the modeling" as the primary means of conveying results to managers. Details on what this will entail are not provided, and overall, it seems that the authors could do more to make the research directly useful and interpretable to managers. For example, a useful elaboration would be to scale up from the 4 case studies to the broader Sierra Nevada/Southern Cascades to predict overall hydrologic effects of climate warming, based on the extent of various geologies, and then to highlight which watersheds are likely to experience the greatest hydrologic changes due to warming. The authors allude to this type of broader analysis, but they do not clearly state that they will complete this work; rather, they imply that their work would facilitate such analysis. Another method of disseminating this research would be for the PI's (or at least the lead PI) to conduct a workshop for water resource planners on the results and implications of this research.</p>
Rating	good

Additional Comments

Comments	<p>The writing is somewhat repetitive; points such as the main hypothesis about geology affecting streamflow regimes are repeated over and over and over. The proposal could have been condensed, allowing additional detail regarding some of the points raised above.</p>
----------	---

Technical Review #2

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team is highly qualified to implement the proposed research, and they have strong track records both collectively and individually. Tague is the developer of the RHESSys model, one of the main tools to be employed here, and will apply many of the same methods she has used in her previous work, suggesting that she will be able to efficiently and effectively lead this project and perform her own sub-tasks. Kirchner, who will complete empirical analysis and climate scenarios, is exceptionally skilled in hydrologic (and other) data analysis. Manga has a strong publication record in the physics of spring-fed systems and has received several prestigious honors for junior scientists. Grant will lend strong insights into linkages between geology and hydrology. The project team appears to have the infrastructure and support structure needed to accomplish this research.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Division of the project up into so many tasks (15) makes the budget presentation somewhat confusing, particularly in terms of indirect costs and summer salary, which constitute the bulk of the requested funding amount. Indirect costs may be double-counted for some items (e.g., Task 1, individual items include indirect costs for UCB & OSU, then indirect costs also listed as separate line item for SDSU Foundation even though no items in Task 1 performed by SDSU personnel). Funding for a course release for Tague
----------	--

Technical Review #2

	(Task 8 is requested; does CalFed funding cover this item? It's hard to keep track of all the summer salaries, but it seems like Godsey's Year 2 summer salary (Task 7, 10) is duplicated. Overall, for a project that employs existing data and models, the requested budget amount is high. It is this reviewer's belief that the proposed research objectives could be achieved for somewhat less than what is requested here.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	The proposed research has merit and would likely contribute valuable knowledge regarding climate-streamflow-geology relations. Primary suggestions for improving this work would be to include a more comprehensive analysis of geologic types, broader assessment of the qualitative and quantitative implications of this work for water supplies draining to the Bay Delta, and more clear methods of communicating results of this work to water planners. The project team includes scientists with outstanding track records who are capable of effectively carrying out the proposed research and, in doing so, of increasing understanding of both basic hydrologic processes in the Sierra Nevada and of how these processes may be affected by future climate change.
Rating	very good

Technical Review #3

proposal title: Does Geology Matter to Streamflow? A geo-climatic assessment of spatial variability in summer flow sensitivity to climate change for San Francisco Bay – Delta system

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The objective is to assess the degree to which differing Sierra Nevada geologies affect groundwater storage, streamflow behavior, and the responsiveness of basins to climate change. The investigators hypothesize that "the future source of water for the Bay-Delta and indeed for all of California under a warming climate is likely to increasingly shift towards the north, where the young volcanic terrains, like those surrounding the Lassen and Shasta volcanic fields, can buffer the effect of diminished snowpack." The proposal logically follows from the clearly stated objectives. The idea is important to long-term water resource planning for California.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The underlying premise that geology partially controls hydrologic behavior of watersheds is well-founded, and the conceptual model is clear and well-justified. The value of the
----------	--

Technical Review #3

	proposed work is the quantification of geologic controls on watershed behavior and the sensitivity of watersheds to various climate change scenarios. Study sites and methodologies are well justified with respect to the project goals. The investigators have done their homework with respect to Sierra Nevada geology and streamflow behavior.
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The approaches have been developed in other studies, and the investigators are well-qualified to do the proposed work. The project will provide novel information, but not novel methodologies or approaches. The approaches have already been tested in other locales. The information is likely to be useful in long-term water resource planning. Essentially, the investigators hypothesize that volcanics, especially young volcanics, act as natural storage reservoirs that damp hydrologic behavior and provide buffering against climate change. The proposed approach will quantify these effects.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Given the experience and capabilities of the investigators, the well-thought out conceptual model,
-----------------	--

Technical Review #3

	and the well-justified methodologies, the likelihood of success is very high.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	This is largely a modeling and data analysis proposal. It will rely on regional climate data and streamflow data previously monitored by USGS. The selection of gages for streamflow analysis has been well thought out with respect to basin geologies and the hypothesized relationships between geology and flow.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The information to be produced will be useful to California water resource managers. The authors propose to develop a GIS-based visualization of modeling scenarios to illustrate the results for environmental managers. Scientists and climate modelers will learn about the work from academic conference presentations and journal articles. However, the dissemination plan is probably the weakest part of an extremely strong proposal.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The investigators are all accomplished scientists and researchers with numerous journal publications on related topics. One of the investigators is the author of the main hydrologic model to be applied to the study watersheds. All of the investigators have managed large funded research projects in the past.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems reasonable and adequate.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	This is an extremely well-written and presented proposal. The conceptual model is clearly presented and defended. Prototypical data analysis is used to justify and explain hypotheses. The relevant scientific literature is well-considered. I was very impressed with the professionalism of this proposal. The investigators seem very knowledgeable of California water resource management issues and the potential role of climate change in altering California's water supply system. The proposed
----------	---

Technical Review #3

	description and quantification of groundwater controls on basin hydrologic behavior does not exist for the Sierra Nevada. This is the best proposal I've read in quite some time.
Rating	excellent